Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1079-RC1, 2020

Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Evaluating Trends and Seasonality in Modeled $PM_{2.5}$ Concentrations Using Empirical Mode Decomposition" *by* Huiying Luo et al.

Anonymous Referee #3

Received and published: 7 June 2020

This manuscript presented an evaluation of the WRF-CAMQ model simulated temporal trends through a detailed comparison with observation using improved CEEMDAN method. The comparison was based on measurements of PM2.5 and its key components, i.e., sulphate, nitrate, ammonium, chloride, organic carbon, and elemental carbon, made at three ground monitoring stations in US from t 2002 to 2008. It is clearly demonstrated that the improved CEEMDAN approach can decompose the observed and simulated temporal trends, which allows to extract more information from the comparisons of individual temporal modes. For example, the authors concluded that the model can better simulate the rate of change of the multi-year trend than the absolute magnitude. At the same time, model can generally reproduce the amplitudes

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Discussion paper

of the sub-seasonal and annual variations for PM2.5, sulphate, and OC. This study revealed that it appears there is a temporal phased shift between the observed and model simulated PM2.5, OC, and EC as large as a half year. It is further suggested that this phase shift indicted "a need for proper temporal allocation of emissions". In general, the manuscript is well organized. This reviewer believes that this is an important work which can potentially help identifying model deficiencies. However, there several concerns needed to be addressed: 1) The authors correctly stated that EMD is a widely used methodology in various field. At the same time, this reviewer would like to suggest that the authors should consider adding some brief high-level descriptions of the method. This will improve the manuscript's readability, especially for those who are not familiar with EMD methods. It is also important to clearly state the criteria how the modes are determined and separated. The statement in line 134-135, "to achieve best mode separation", leaves much room for interpretation. The discussion on determination of tp and tm is interesting and thorough. It does, however, leave an impression that the evaluation of tp and tm is somewhat uncertain and is not completely deterministic. This reviewer would like to suggest adding additional text to discuss if the determination of tp and tm is sufficiently accurate or useful for model assessment to identify issues in the processes at the similar time scale as decomposed tp and/or tm. This will strengthen the manuscript to demonstrate the usefulness of the improved CEEMDAN approach in model assessments. 2) Section 2 (starting from line 74) provided a good discussion on how the observation data sets are selected. It is equally important to discuss the temporal resolution of model in terms of the driving factors, e.g., emissions. This will give readers a sense if one should expect if the model should reproduce observations at certain temporal scale. For example, if the emissions are given in yearly average, one would consider the impact of the lack emission temporal variability on the comparison of seasonal and/or sub-seasonal trends. 3) This reviewer believes that the concluding remark of "indicating the need for proper allocation of emissions" is an important conclusion. However, it was not adequately justified. There are many controlling factors and processes. The authors should have provided more discussions to illustrate how they narrowed to emissions as the likely factor. It should also be pointed out that SOA is typically a large component of OC. Changes in emissions to affect OC will likely have implications on O3. 4) The authors presented detailed trend analysis on PM2.5 and its components. It is also scientifically interesting to understand the relative contribution of each component and their contribution to the identified temporal variability, which are useful to gain insights into controlling factors. This reviewer would like to suggest the authors to consider addition of the trend analysis on the relative contribution of sulphate, nitrate, ammonium, organic carbon, and elemental carbon to PM2.5. More specific to the manuscript, it would be much easier to interpret the results shown in Table 1, 2, and 3 if the relative contribution of each component is known. 5) In general, model evaluation is designed to improve model. It is difficult to relate the comparison results presented in this manuscript to specific model deficiencies in description of the chemical/physical processes and/or issues in model data sets, meteorological field and/or emission data. As sulphate, OC, nitrate are controlled by very different chemical processes, this reviewer would like to encourage the authors to further explore the difference in the comparison results for these species, which may reveal additional insights into the process-level model deficiencies.

Specific Comments: 1) Figure 3 is hard to read because of log-log scale. It may be better to change the x-axis to the IMF number and y-axis to the ratio between model and observation characteristic scales. A second y-axis can be added to show the absolute characteristic scales for each IMF. 2) Section 4.2. Figure 6 shows some variation in time-derivatives. At the same, this reviewer would like to argue that about half of cases shown in the figure can be well approximated by linear assumption. The authors should comment on this aspect.

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